expenses and 25 francs a day each during its sessions in Rome, or other fixed place of meeting.

The constitution of this Board is admirable, since it secures a fair representation of the leading scientific men of Italy. The Italian Government does not, it seems, refer such questions exclusively to one individual, but endeavours to obtain a consensus of the scientific opinion of the country.

Hôtel de la Ville, Florence E. RAY LANKESTER

Fabry's Comet and Barnard's Comet

NOT having seen any mention of the rapid apparent growth of the tail of Fabry's comet, probably some of your readers are not aware to how great a length it extended. On April 26 occurred the first fine night after a very unusual series of overcast ones, and about 14h. G.M.T. I was surprised to see the tail reaching up to, or at least to within 1° of, δ Cassiopeiæ, a distance of 38° from the place given in the ephemeris for the nucleus, which was far below the horizon; and the tail would doubtless have been visible to a greater distance but for the brightness of the Milky Way. The following night, about 10h., it reached at least up to the Cluster in Perseus, a distance also of 38° from the predicted position of the nucleus; it was very narrow both nights. The next night, which was pretty fine, I failed to find any trace of the tail.

The principal tail of Barnard's comet is also very narrow: on May 1 its length was 4½°, as seen with a pair of field-glasses. With the telescope this comet had also a faint tail *nf*, about 16′ long, making an angle of 65° or 70° with the other.

Sunderland, May 7 T. W. BACKHOUSE

"Pumice on the Cornish Coast"

STEAMER-CINDERS, similar to those referred to by Mr. Whitaker in NATURE for April 29 (p. 604), occur frequently on the Falmouth beaches; but as there seemed to me little probability of their being mistaken for pumice, I did not refer to the matter in my communication to your columns (April 15, p. 559).

Mr. Murray tells me that the pumice I found is felspathic, and that from its form and diminished buoyancy it had evidently been a long time in the water. The fragment was sent by him to Mr. Whitaker, who at once recognised its true character and its distinction from the steamer-cinders observed by him on the Suffolk coast, one of which he sent to Mr. Murray to satisfy him as to their very evident source. H. B. GUPPY

95, Albert Street, Regent's Park, N.W., May 8

THE VELOCITY OF LIGHT

I.

[A reinvestigation of this important constant has recently been published by Prof. Newcomb. Before we state his methods and results we think it well to reproduce the following admirable historical notice with which his monograph commences.—ED.]

WHEN it became clearly understood that vision was not an immediate perception of objects by the eye, but was produced by the passage of an entity called light from the object to the eye, the question of the time which might possibly be required for this passage became one of interest to physical investigators. The first proposal for an experimental investigation of this question is due to Galileo.[1] He suggested that two observers, each holding a lantern, should be stationed at a distance apart, in sight of each other. Each should be supplied with a screen, by which he could, in a moment, cover or uncover his lantern. One observer should then uncover his lantern and the other uncover the other the moment he perceived the light from the first lantern. The interval which elapsed after the first uncovered his light, until he perceived the light of the second, would be the interval required for the light to go and come, plus the time required for the second observer to perceive the light and make the required movement. This experiment was tried by the Florentine Academy, and of course resulted in a conclusion that the time required was insensible, since we now know that it was far below any interval that could have been detected by so rude a method.

It is, however, interesting to notice that, rude though this experiment was, the principle on which it was based is the same which underlies one of the most celebrated methods used in recent times for the attainment of the same object. Two very simple improvements which we might have imagined the Academicians to make in their experiments are these:—

Firstly, to dispense with the second observer, and in his place to erect a mirror, in which the first observer could see the image of his own lantern by reflection. The time required for the second observer to perceive the light and uncover his lantern would then have been eliminated from the problem. The interval sought would have been that between the moment at which the observer uncovered his lamp and the moment at which he perceived the reflection.

Secondly, to use the same screen with which he uncovered his own lamp, to cut off the returning ray from the distant mirror, and thus obviate the necessity of an uncertain estimate of the interval between his muscular effort in removing the screen and his perception of the return flash of light. If the image was perceived before he could cover his own eye with the screen removed from the lamp, it would show that the interval of passage was less than the time required to make a motion with the screen. This interval might have been reduced almost indefinitely by having both lines of sight as near together as possible.

Had these improvements been made, the Academicians would have had, in principle, Fizeau's method of measuring the velocity of light by the toothed wheel, a tooth being represented by the screens. To realise the principle more fully, the two lines of sight should have been rendered absolutely coincident by reflection through a telescope. It does not, however, appear that any effort to put the question to a severer test was made until the subject was approached from a different point of view. It was probably considered that the passage was absolutely instantaneous, or, at least, that the velocity was above all powers of measurement.

The subject was next approached from the astronomical side. In 1676 Roemer made his celebrated communication to the French Academy, claiming that observation of the eclipses of the first satellite of Jupiter did really prove that light required time to pass through the celestial spaces.[1] He found 11m. to be the time required for light to pass over a distance equal to the radius of the earth's orbit. Dominique Cassini, while admitting that the hypothesis of Roemer explained the observed inequality, contested its right to reception as an established theory, on the ground that the observed inequality might be a real one in the motion of the satellite itself.[2]

Continued observation showed that the time assigned by Roemer for the passage of light between the earth and sun, or "the light equation" as it is briefly called, was somewhat too great. In 1809 it was fixed by Delambre at 493·2s., from an immense number of observations of eclipses of Jupiter's satellites during the previous 150 years. This number has been received as a definitive result with a degree of confidence not at all warranted. In 1875, Glasenapp, then of Pulkowa, from a discussion of all available eclipses of Jupiter's first satellite between 1848 and 1870, showed that results between 496s. and 501s. could be obtained from different classes of these observations by different hypotheses.[3]

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[1] Poggendorff, *Geschichte der Physik*, p. 402, where reference is made to the *Saggi* of the Florentine Academy.

[1] *Paris Memoirs*, tome i. p. 212, and tome x. p. 575.

[2] *Ibid.* tome viii. p. 47. Poggendorff (*Geschichte der Physik*, p. 656) quotes Maraldi as also contesting Roemer's explanation on the ground that a similar inequality should be found depending on the position of Jupiter in his orbit. The ground here taken was quite correct, the only fallacy being the assumption that such an inequality did not exist.

[3] This paper of Glasenapp's was published only in the Russian language as an inaugural dissertation, and in consequence has never become generally known.

As not a trace of Delambre's investigation remains in print, and probably not in manuscript, it is impossible to subject it to any discussion.[1]

The discovery of aberration by Bradley afforded an independent and yet more accurate method of determining the light equation. We call to mind that the latter constant, and that of aberration, are not to be regarded as independent of each other, but only as two entirely distinct expressions of the same result. The constant of aberration gives a relation between the velocity of light and the velocity of the earth in its orbit from which, by a very simple calculation, the time required for light to pass from the sun to the earth may be deduced.

It is remarkable that the early determinations of the constant of aberration agreed with Delambre's determination of the light equation, although we now know they were both in error by an amount far exceeding what was, at the time, supposed probable. Struve's value, 20″·445, determined in 1845 from observations with the prime vertical transit of Pulkowa, has been the standard up to the present time. The recent determinations by Nyrén being founded on a much longer series of observations than those made by Struve, and including determinations with several instruments, must be regarded as a standard at present. His result is :—[2]

Definitive value of the constant of aberration = 20″·492 ± 0″·006.

At the time Struve's result was published there was an apparent difference of 1 per cent. between its value and that of the light equation determined by Delambre. The question then naturally arose whether the light equation, deduced on the hypothesis that the tangent of the angle of the constant of aberration was the ratio of the velocity of the earth in its orbit to the velocity of light, might not need correction or modification. This question cannot yet be considered as definitely settled, since the modifications or corrections might arise from a variety of causes. One of these causes is connected with a very delicate question in the theory of the luminiferous medium; a question which can be most clearly understood when placed in the following form :—It is a result of optical principles that a ray falling perpendicularly upon the bounding surface of a refracting medium retains its direction unaltered. Now, if this surface is carried along by the motion of the earth, and the light comes from a star, and it is desired that this surface shall be so directed that there shall be no refraction, must it be placed perpendicular to the *true* direction of the star as freed from aberration, or to its *apparent* direction as affected by aberration? The difference of the two directions may exceed 20″, and since the index of refraction of glass exceeds 1·5, there will be a difference of more than 10″ in the direction of the refracted ray, according as we adopt one or the other hypothesis. Assuming that the standard direction would be perpendicular to the true or absolute direction of the star, it is easily shown that the constant of aberration determined in the usual way would be too large by a quantity depending on the ratio of the thickness of the objective to the focal length of the telescope. In an ordinary telescope the difference would be nearly one-hundredth of the total value of the aberration, and would, therefore, closely correspond to the discrepancy between Delambre's result from the satellites of Jupiter and the modern determinations of the constant of aberration. The question of this particular cause was set at rest by Airy's experiments with a telescope filled with water, which showed that the result was independent of the thickness of the objective, and, therefore, that the apparent direction of the star was that on which refraction depended.

If, in accordance with the undulatory theory of light, we suppose the hypothetical entity called "the luminiferous medium" to be a substance, each part of which has its own definite and fixed location in space, then we must conceive that another unknown quantity may enter into the problem, namely, the motion of the heavenly bodies through this medium. We have relative motions in the solar system, exceeding 50 kilometres per second, and possibly greater relative motions among the stars. Now it is clear that the heavenly bodies cannot all be at rest relative to the medium, but must move through it with velocities at least of the order of 50 kilometres per second, and possibly greater without limit, since it is conceivable that the whole visible universe might be moving in a common direction relative to the medium.

It is easily seen that if we suppose the velocity of the earth, through the medium, to have a small ratio, a, to the velocity of light, then the observed constant of aberration may be altered by an amount found by multiplying its value by a quantity of the order of magnitude of a. This alteration would be entirely insensible if the earth does not move through the medium with any greater velocity than it does around the sun, since the value would then be only $\frac{1}{10000}$. It is remarkable that so far as yet investigated every optical effect arising from such a motion, which could be measured on the surface of the earth, is of the order of magnitude of the square of a. Thus, no phenomenon has yet been discovered which can be traced to the motion in question.

Assuming that there is no general motion of the solar system through the ether of a higher order of magnitude than that of the relative motions of the fixed stars to each other, and that the ordinary theory of aberration is correct, there will be three constants between which a relation exists, such that when any two are found the third can be determined. These constants are :—

1. The distance of the sun in terrestrial units of measure;
2. The velocity of light in units of the same measure; and
3. The constant of aberration, or, which is supposed to be equivalent, the light equation.

Until our own time the first and third constants were used to determine the second. From the fact that light required about 500 seconds to traverse the distance from the sun to the earth, and that the distance of the sun was, as supposed, 95,000,000 of miles, it was concluded that light moved 190,000 miles per second. The hopelessness of measuring such a velocity by any means at the command of physicists was such that we find no serious attempt in this direction between the date of the futile effort of the Florentine Academy, and that of the researches of Wheatstone, Arago, Fizeau, and Foucault nearly two centuries later. One of the most curious features presented by the history of the subject is that two entirely distinct methods, resting on different principles, were investigated and put into operation almost simultaneously. The revolving mirror of Wheatstone, and its application to determine the duration of the electric spark and the velocity of electricity, come first in the order of time. But, before this ingenious instrument had been applied to the actual measurement of the velocity of light, Fizeau had invented his toothed wheel, by which the same object was attained.

Fizeau's paper on the subject was presented to the Academy of Sciences on July 23, 1849.[1] We have already shown that his method and that of Galileo rest fundamentally upon the same principle. The arrangement of his apparatus was substantially as follows :—

A telescope was fixed upon a house at Surésne pointing to the hill Montmartre. On this hill was a second fixed telescope looking directly into the first, the distance between them being about 8633 metres. In the focus of this second telescope was fixed a small reflector, so that a beam of light from the first would be reflected directly back to it. By means of a transparent glass, fixed in the eye-piece at an angle of 45°, a beam of light was sent from the first telescope to the second, and, on its return through a total distance of 17 kilometres, could be seen as a star by an eye looking through the first. Alongside the eye-piece of the latter a revolving wheel, with 720 teeth cut upon its circumference, was fixed in such a way that the beam of light both in going and coming had to pass between the teeth. When the wheel was set so that the tooth was in the focus, the beam would be entirely cut off in its passage through the telescope. Changing the position of the wheel through half the space between the middles of two consecutive teeth, the light would go and come freely between the teeth. When the wheel was set in revolution a succession of flashes would be sent out. If, on the return of each flash, a tooth was interposed, it would be invisible to the eye looking through the telescope. Fizeau found that with a velocity of 12·6 turns per second each flash which went out was on its return cut off by the advancing tooth. With a velocity twice as great as this it was seen on its return through the opening next following that through which it went. With three times this velocity it was caught on the second tooth following, and so on.[1]

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[1] The author could find no remains of this investigation among Delambre's papers at the Paris Observatory.

[2] *Mémoires de l'Académie Impériale des Sciences de St. Pétersbourg*, vii. série, tome xxxi. No. 9.

[1] *Comptes rendus*, vol. xxix. 1849, p. 90.

This experiment of Fizeau was soon followed by the application of the revolving mirror of Sir Charles Wheatstone. Shortly after measuring the duration of the electric spark this investigator called attention to the fact that the same system could be applied to determine the velocity of light, and especially to compare the velocities through air and through water. In 1838 the subject was taken up by Arago, who took pains to demonstrate that it was possible, by the use of the revolving mirror, to decide between the theory of emission and that of undulations by determining the relative velocities in air and in a refracting medium.[2]

The difficulties in the way of securing the necessary velocities of the mirror and of arranging the apparatus were such that Arago never personally succeeded in carrying out his experiments. This seems to have been done almost simultaneously by Foucault and Fizeau about the beginning of 1850. Both experimenters seem to have proceeded substantially on the same principle and to have reached the same result, namely, that the motion of light through water was slower than through air in the inverse proportion of the indices of refraction of the two media.[3]

An important and most necessary modification of Arago's plan was made by these experimenters. As originally proposed, the plan was to send an instantaneous flash of light through water and through the air, and to receive it on the revolving mirror and determine the relative deviations in the positions of the images produced by the two rays. This system would, however, be inapplicable to the measurement of the actual time of transmission, owing to the impossibility of making any comparison between the time at which the flash was transmitted, and that at which it was received on the mirror. This circumstance would, indeed, have rendered the actual realisation of Arago's project nearly impossible for the reason that the flashes of light, seen through the water, would have reached the mirror at every point of its revolution; and only an exceedingly small fraction of them could have been reflected to the eye of the observer.

This difficulty was speedily overcome by Foucault and Fizeau by a most ingenious arrangement, of equal importance with the revolving mirror itself. Instead of sending independent flashes of light to be reflected from the mirror, a continuous beam was first reflected from the revolving mirror itself to a fixed mirror, and returned from the fixed mirror back on its own path to the revolving one. A succession of flashes was thus emitted as it were from the fixed mirror, but their correspondence with a definite position of the revolving mirror was rendered perfect. Moreover, by this means, the image was rendered optically continuous, since a flash was sent through and back with every revolution of the mirror, and after the velocity of the latter exceeded 30 turns per second, the successive flashes presented themselves to the eye as a perfectly continuous image.

It was not until 1862 that this system was put into operation by Foucault for the actual measurement of the velocity of light through the atmosphere. A new interest had in the meantime been added to the problem by the discovery that the long-accepted value of the solar parallax was too small, and that the measurement of the velocity of light afforded a method of fixing the value of that constant. The central idea of the method adopted by Foucault was that already applied in comparing velocities through different media. The element sought is made to depend upon the amount by which the revolving mirror rotates while a flash of light is passing from its surface to the distant reflector, and coming back again. As the details of Foucault's method will be best apprehended by a comparison of them with those adopted in the present investigation, a complete description of his apparatus will here be passed over. It may, however, be remarked, that what he sought to observe was not the simple deviation of a slit, but the deviation of the image of a reticule. The deviation actually measured was 0·7 millimetre, and the system adopted was to determine at what distance, with a definite velocity, this amount of deviation could be obtained. His result for the velocity of light was 298,000 kilometres per second.

The next measures of the element in question were those of Cornu. The method which he adopted was not that of the revolving mirror, but Fizeau's invention of the toothed wheel. His earlier measures, made in 1870, and communicated to the French Academy in 1871, led to a result nearly the same as that of Foucault.[1] This result was, however, not so satisfactory that the author could record it as definitive. He, therefore, in 1874, repeated the determination on a much larger scale and with more perfect apparatus. The distance between the two stations was nearly 23 kilometres, and therefore much greater than any before employed. He was thus enabled to follow the successive appearances and extinctions of the reflected image to the thirtieth order; that is, to make fifteen teeth of his wheel pass before a flash returned from the distant reflector, and to have it stopped by the sixteenth tooth.

This method has a defect, the result of which is evident by an examination of Cornu's numbers. It is that the extinctions and reappearances of the light as the wheel changes its speed are not sudden phenomena, occurring at definite moments, but are so gradual that it is difficult to fix the precise moment at which they occur. Of this defect the able experimenter was fully conscious, and his discussion of the disturbing causes which come into play, and of the amount of error due both to the apparatus, the observer, and to the method of eliminating them, form altogether one of the most exhaustive discussions of a physical problem.[2] But the uncertainties are not of a kind which admit of complete investigation, and it now appears that although his result was far superior in point of accuracy to that of Foucault, it was nevertheless in error by about 0·0015 of its whole amount. It was, in fact, when reduced to a vacuum, 300,400 kilometres per second, while we may now regard it as well established that the true velocity is less than 300,000.

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[1] It is curious that the author's account of this remarkable experiment, which forms an epoch in the history of physical science, is contained within the limits of two pages, and terminates without any definite discussion of the results. It is merely stated that the result is 70,948 leagues of 25 to the degree, but the velocity, in kilometres, which must have been that first obtained, is not given, nor is it stated what length the degree was supposed to have in the computation.

[2] *Comptes rendus*, 1838, vol. vii. p. 954; *Œuvres de François Arago*, vol. vii. p. 569.

[3] *Comptes rendus*, xxx. 1850, pp. 551 and 771.

[1] *Comptes rendus*, vol. lxxiii. 1871, p. 857.

[2] Annales de l'Observatoire de Paris, *Mémoires*, tome xiii.

The next determination of the velocity of light was that of Michelson,[1] whose result was 299,910 kilometres per second. His investigation being a part of the first volume of the present series need not be here discussed, but it is worth while to remark that his method seems far superior in reliability to any before applied.

An attempt has been made by Messrs. James Young and George Forbes to improve Fizeau's method, by diminishing the uncertainty arising from the gradual extinction of the visible image.[2] By the method of these experimenters the result depends, not upon the moment when the image disappears, but when two images, side by side, are equal in brightness. This is effected by employing two reflectors, at unequal distances, but nearly in the same line from the telescope, to return the ray. Each reflector then forms its own image in the field of view of the sending telescope. With a regularly increasing velocity of the toothed wheel, each image goes independently through the same periodic series of changes as when only one mirror is used; but owing to the unequal distance the period is not the same. If the speed of the mirror be carried to such a point that the difference of phase in the two images is half a period, then one image will be increasing while the other is diminishing, and the stage at which the two images are equal would appear to admit of fairly accurate determination.

The distant reflectors were separated from the observing telescope by the Firth of Clyde. The distances were respectively 16,835 feet, and 18,212 feet. A study of the printed descriptions of their experiments gives the impression that the performance of the subsidiary parts of the apparatus was not such as to do justice to the method. The resulting velocity of light was 301,382 kilometres per second, and the difference between the extreme results of twelve separate determinations was 4000 kilometres.

The most important result of the work of these gentlemen, could it be accepted, would be the establishment of a difference between the velocities of differently-coloured rays. We may regard it as quite certain, from the absence of any change in the colour of the variable star, β Persei, while it is increasing and diminishing, that the difference between the times required by red and by blue rays to reach us from that star cannot exceed a moderate fraction of one hour. It is quite improbable that its parallax is more than $0''{\cdot}1$, and therefore probable that its distance is 2,000,000 or more astronomical units. The possible difference between the velocities in question can, therefore, only be a small fraction of the hundred-thousandth part of either of them. No apparatus yet devised would suffice for the measurement of a difference so minute, and we are justified in concluding that the phenomena observed by Messrs. Young and Forbes arose from some other cause than a difference between the velocities of red and blue rays.

The present determination had its origin as far back as 1867. In his "Investigation of the Distance of the Sun," published in that year, the author introduced some remarks upon Foucault's method, and pointed out the importance to the determination of the solar parallax of repeating the determination of Foucault on a much larger scale, with a fixed reflector placed at a distance of three or four kilometres.[3]

From that time forward the subject excited the attention of American physicists, several of whom formed plans, more or less definite, for executing the experiments. As, up to the year 1878, no important steps in this direction had been taken, the author, in April of that year, brought the subject before the National Academy of Sciences, with the view of eliciting from that body an expression of opinion upon the propriety of asking the Government to bear the expenses of the work. The subject was referred to a Select Committee, who, in January, 1879, made a favourable report on the subject, which was communicated to the Secretary of the Navy. On the recommendation of the Secretary, Hon. R. W. Thompson, Congress, in March following, made an appropriation of $5000 for the purpose, and the author was charged by the Department with the duty of carrying out the experiments.

In the meantime it became known that Mr. Michelson had made preparations for repeating Foucault's determination at his own expense, with the desirable improvement of placing the fixed reflector at a considerable distance. But before the reliability of Mr. Michelson's work had been established, the preparations for the present determination had been so far advanced that it was not deemed advisable to make any change in them on account of what Mr. Michelson had done. The ability shown by the latter was, however, such that, at the request of the writer, he was detailed to assist him in carrying out his own experiments, and acted in this capacity until September 1880, when he accepted the Professorship of Physics in the Case Institute, Cleveland, Ohio. After the departure of Mr. Michelson his place was taken by Ensign J. H. L. Holcombe, U.S.N., who assisted in every part of the work to the entire satisfaction of the projector until its close.

PANCLASTITE

DR. SPRENGEL has sent us a reprint of a note sent by him to the *Chemical News* on this subject. After showing that these new explosives, so named by Mr. Turpin, are not original, he continues:—

"The 'beau idéal' of a detonating explosive is a mixture of 8 parts (88·9 per cent.) of liquid oxygen and 1 part (11·1 per cent.) of liquid hydrogen.

"In my paper of 1873 I say, p. 799:—'On referring to the foregoing table the reader will be reminded that peroxide of hydrogen is the highest oxygen compound known, while nitric anhydride is the compound which contains the largest amount of oxygen available for combustion (74 per cent.). But as this compound, as well as the next two, nitric peroxide (69·5 per cent. oxygen) and tetranitromethane (65·3 per cent. oxygen) are : at present : on account of their nature and their difficult preparation, mere chemical curiosities, my attention naturally turned to the fourth, to *nitric acid* (63·5 per cent. oxygen), which is a cheap and common article of commerce.'

"Now, when Mr. Turpin's attention turned to the second oxidiser on my list—to nitric peroxide—he found that this substance does *not corrode* metals, such as iron, copper, and tin under 356° F. (180° C.); and further, that combustible liquids, such as petroleum, carbon bisulphide, and nitro-benzene are readily soluble in nitric peroxide *without* rise of temperature. These are valuable properties, *first noticed by Mr. Turpin.*

"What was formerly a chemical curiosity is now an article of commerce. Nitric peroxide may be bought to-day at eighteenpence the pound, and I see ways and means of producing it a great deal more cheaply. Nitric peroxide is a yellowish liquid, heavier than water (sp. gr. = 1·451), and boils at 71° F. (22° C.), but may be kept like ether or similar volatile liquids. In France it is sent about in tinned-iron cans.

"Taking as a typical example a benzene-mixture—

$$\begin{array}{rl}
C_6H_6 = 18{\cdot}4 = & \left.\begin{array}{r} 17{\cdot}0\ C \\ 1{\cdot}4\ H \\ 56{\cdot}8\ O \end{array}\right\} = \begin{array}{l} 62{\cdot}6\ CO_2 \\ 12{\cdot}6\ H_2O \end{array} \\
7\tfrac{1}{2}(NO_2) = \underline{81{\cdot}6} = & 24{\cdot}8\ N \\
100{\cdot}0 &
\end{array}$$

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[1] "Astronomical Papers of the American Ephemeris," vol. i. part iii. Owing to an error in applying one of the corrections the result was given as 299,942 kilometres.

[2] *Philosophical Transactions* for 1882, p. 231.

[3] Washington Observations, 1865. Appendix ii.